LETTERS TO THE EDITOR.

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Lord Kelvin on the Origin of Granite.

IN 1897 Lord Kelvin delivered an address to the Victoria Institute on the age of the earth, Sir G. G. Stokes being in the chair. Incidentally, the address included theories of the origins of granite, basalt and continents, and touched on the question of the inclusion of gases in various rocks.

At the late meeting of the British Association two sectional Presidents referred to Lord Kelvin's theory, both apparently accepting his lordship's conclusions without noticing his premisses.

My old master, William Pengelly, used to teach that as every theory depended on many facts, if but one fact were disproved the theory fell to the ground. From this aspect there are several points in Lord Kelvin's theory which seem to require elucidation.

Lord Kelvin starts from a period in the earth's evolution when a lava ocean forty kilometres deep covered a solid nucleus. The specific gravity of the liquid lava is assumed to be 2·50. It is also assumed, on what seem firm grounds, that solid lava would sink in fluid lava, and on more doubtful grounds, that all minerals crystallising out of lava would also sink. On this assumption the lava ocean silts up, and the surface does not "freeze" until the forty kilometres of the crust (excepting the future ocean basins) are composed of solid crystals set in an interstitial mother liquor.

According to Lord Kelvin, continents arose from the drifting and unequal distribution of the crystals falling like a "snow shower" through the lava; the ocean basins arising from the contraction of the mother liquor in cooling. The theory is brilliantly unfolded, but there are many technical difficulties, *e.g.* Lord Kelvin's granite, besides being an exclusively primeval rock, is composed of drifted crystals of felspar, mica, quartz and hornblende (or some of them) set in a basaltic matrix. The ultimate mother liquor is made to serve the purpose both of basalt and of the matrix of granite. One gas at least, viz. $C.O_2$, treated as original in basalt, might well arise from the calcite which so often occurs in that rock. But the most serious and far-reaching difficulty is involved in the specific gravity of lava minerals. Assuming the liquid lava to be 2·50, there are several minerals which would float in such an ocean, and if there were but one, it would suffice to provide a floating crust or slag which would blanket the glowing lava and entirely upset all heat calculations based on the consolidation of the earth from within to without.

What perplexes me in the matter is that so many philosophers who accept Lord Kelvin's conclusions hold themselves at liberty to reject his premisses.

For instance, the President of Section E, while declaring his adhesion to Lord Kelvin's time-views as against the geologists, entirely ignores both his continental theory and the main premiss as to the specific gravity of the lava ocean. Lord Kelvin assumes the lava to be 2·50, while Sir John Murray assumes the crust to be 2·50, a most fundamental distinction.

Petrologists have fought desperately over the question of the origin of granite, but so far as I am aware they are agreed on all the main points.

I believe myself that every known fact fits most exactly into the grand working hypothesis that granite is a plutonic rock formed by hydrothermal action and pressure out of a previously existing rock, which consisted in the first place of those light aluminous soda and potash silicates which first consolidated on the surface of the primeval lava ocean. To these silicates we have but to add water, in order to form (so far as constituents go) a typical muscovite granite—absolutely nothing more; indeed, less, as we may omit the soda. The constituents of muscovite ($K_2O,2H_2O,3Al_2O_3,6SiO_2$) are simply leucite ($K_2O,Al_2O_3,4SiO_2$) and water. Introduce sodalite, an even more likely constituent of the primeval crust than leucite, and we have all our materials for ordinary granites, except magnesia, with which, however, Lord Kelvin supplies us by means of eruptions of the basaltic mother liquor.

This is but a working hypothesis, but it will work; whereas Lord Kelvin's novel theory throws the whole problem of granite into inextricable confusion, even starting from the hopeless position of disagreement as to what the term granite denotes, geologically, mineralogically and petrologically.

The whole question seems to turn on one single point, viz. whether the first primeval crust when cold floated on the subjacent lava. According to the elements of mineralogy it would do so; but many physicists have assumed that upon consolidation it would sink. Will not some of the distinguished specialists in mineralogy and petrology pronounce judgment on this question, which is really troubling unlearned and ignorant men who are genuinely seeking information? The conflict of authority is quite overwhelming. A. R. HUNT.

Torquay, February 13.

Effects of Lightning upon Electric Lamps.

I HAVE often seen luminous trails, similar in appearance to those shown in Mr. Webb's photographs (p. 343), in photographs taken at night. That there are any effects in these or Mr. Webb's pictures that cannot be explained by a moving camera, I am unable to convince myself of. The identical form of the discharges from different lamps has been explained by the distance of the discharge causing them. Granting that it is possible to have a discharge, so intricate in character, exactly duplicated at a second lamp (which is scarcely conceivable), their magnitudes in the pictures should be inversely proportional to their distances. But we find that, in the pictures, the scrawls are all of the same size. A lamp close to the camera, and a distant lamp, show the trails *on the same scale.*

The beading of the trails can be easily explained by the alternations of the current, the carbons fluctuating in brilliancy. I am informed that in Dover these periodic fluctuations are very conspicuous. Where a trail turns suddenly, the beads are closer together, due to the motion of the camera being slower when the direction of motion is about to change.

If I remember right, there are one or two cases where we have a very large and brilliant pattern, and several similar ones on a smaller scale. This could be explained by reflection from the inner surfaces of the lens. R. W. WOOD.

London, February 20.

The Fitting of the Cycle to its Rider.

I HAVE read Mr. Hutchins's communication (p. 368) with considerable interest. Mr. Hutchins is at the head of the Forest Department which has been recently established by the Cape Government, and the improved method of riding that he has adopted, in accordance with the views expressed in my recent paper, have evidently been of service to him in traversing the very rough country to enable him to carry out his duties. Mr. Hutchins's experiences so closely agree with my own that I can say little in criticism of his letter. I think, however, that he will find that the gain from the lengthened crank advocated by me cannot be explained by the very simple formula that he gives. My son and I went very carefully into this matter at the time I prepared my paper, and I think if Mr. Hutchins refers to it he will find, if we consider the mechanical advantage apart from the gain in nerve waste, the mathematical analysis of ankling given by lengthened crank both give the only explanation which would be satisfactory to a mathematician. In our explanation the weight of the leg plays a very important part, and it follows therefore that a heavy legged man gets most from our system of riding. R. E. CROMPTON.

Crompton Laboratory, Kensing on Court, London, W.

THE point raised by Mr. Hutchins in his letter (p. 368) is worth considering, but Mr. Crompton, who by his wonderful riding has done so much to popularise the use of the long cranks first suggested by Mons. Boulay, is not heavily built. A man who is over fifty cannot move his legs so quickly as when he was younger; and so middle-aged persons, stout or slim, profit greatly by using long cranks and high gears. Most people when in a hurry run upstairs two steps at a time, and bicyclists, whether in a hurry or not, find it an advantage to raise gears and lengthen cranks.

The question is, how far may we go without unduly increasing the weight of our bicycles?

My age is fifty-one, weight 15 stone, height under 6 ft., and,

having tried everything between 5¼ in. cranks with a 54 in. wheel, and my present machine with 10½ in. cranks and 108 in. gear, I can now travel greater distances and climb steeper hills with less effort on a 42 lbs. bicycle than when riding a 28 lbs. machine fitted with 6½ in. cranks and 66 in. gear. I intend to try 11 in. cranks and 120 in. gear, but this necessitates my getting a longer and heavier machine, and it is probable that I shall lose as much as I gain.

The extraordinary ankle-play developed by long-crank men improves their walking; and, after a long hard ride, the difference between their swinging elastic step and the muscle-bound hobble of the short-crank riders is very striking.

Instead of Crompton foot-plates I have just fixed a flanged clip to each pedal, so that the inner edges of the soles of my shoes can be pushed under the clips; and they are almost as comfortable and efficient as the "Otto" straps of years ago.

WM. H. MASSEY.

Twyford, Berkshire, February 17.

Indian Corn.

IN the "Encyclopædia Britannica," vol. xv. p. 309, it is stated that no mention was ever made of maize by Eastern travellers in Africa or Asia prior to the 16th century A.D. Slight doubts about this statement have occurred to my mind lately, while I was reading the Hakluyt Society's "India in the Fifteenth Century." There, in the English translation by the late Count Wielhorsky of the "Travels" of Athanacius Nikitin, the Russian, whose Eastern travels took place about 1470-1474, when the work was written by himself, we read concerning the Indians: "They live on *Indian corn*, carrots with oil, and different herbs" (p. 17). Has this mention of the cereal any weight to countenance the theories which seek to assert that maize was known in the East before the discovery of the Western Continent? Or, does what is meant or translated by the word *Indian corn* here differ materially from *Zea Mays?*

Apropos of these queries, I may mention that A. de Candolle is in error in his post-dating the introduction of maize into Japan on the sole ground that Kaempfer (who was there during 1690-92) does not mention it.[1] According to a native work (Kikuoka, "Kindai Sejidan," 1733, lib. 2, § 4), maize was introduced into the islands about the beginning of the period of Tenshô (1573-91). After Sweet Sorghum (*Sorghum saccharatum*), of earlier introduction with the name *Morokoshi-Kibi* (*i.e.* Chinese-Millet), maize was called *Tô-Morokoshi* (*i.e.* Chinese Chinese-Millet) in the eastern provinces, where, of course, its propagation followed that in the western parts. In the dialect of the latter, where the people were more directly concerned with its introduction, maize was named *Nanban-Kibi*, or Millet of the Nambans (Spaniards and Portuguese), who were entirely excluded from the empire since 1639, which thus would stand as latest possible date of the introduction.

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The Production of Electrolytic Copper.

IN a note on the production of electrolytic copper, on p. 371 of NATURE of February 15, it is stated that "Mr. S. Cowper-Coles has hit upon a new plan, in which the copper is deposited on a vertical mandril, which is caused to rotate at a very rapid rate. . . . As a consequence, a smooth and dense deposit has been obtained with current densities approaching 200 ampères per square foot." In reference to this I should like to point out that the idea of rotating the kathode with a view to obtaining greater rates of disposition is an old one. We have had in use at the Owens College for the last seven years a copper depositing tank in which the kathode consists of a vertical mandril 9 inches long and 3 inches diameter kept in rapid rotation, and capable of receiving a good copper deposit with a current of 100 ampères. The arrangement was devised by Mr. Henry Wilde, F.R.S., to illustrate the working of his patent, No. 4515, of 1875, and differs little from that used by Mr. Cowper-Coles. The mandril is driven from above, so that a stuffing-box in the bottom of the tank is unnecessary. CHARLES H. LEES.

The Owens College, Manchester, February 19.

[1] "Origin of Cultivated Plants," p. 392.

THE WEST INDIAN AGRICULTURAL CONFERENCE.

THE second West Indian Agricultural Conference was opened on January 6 in the hall of the House of Assembly, Barbados, under the presidency of Dr. D. Morris, C.M.G., Imperial Commissioner of Agriculture for the West Indies. There were forty representatives present, including the heads of all the Botanical, Chemical and Educational Departments, as well as the representatives of the principal Agricultural Societies in the West Indies. Some of these gentlemen had journeyed for the best part of a week to take part in the two days' labour that awaited them.

The representatives were received in the hall of the House of Assembly at 10.30 a.m. by his Excellency Sir James Shaw Hay, the Governor, who opened the conference with a short address of congratulation to the Department of Agriculture, and of welcome to the visitors from other Colonies.

The President then delivered his address, which summarised the work done by the Imperial Department during the preceding year, and alluded to some of the problems which were expected to be discussed at the conference: the best seedling canes and their record, Imperial aid for co-operative central factories, reducing cost of cultivation, subsidiary industries which have done so much for Jamaica, Trinidad and Grenada, agricultural education and treatment of diseased plants, were subjects that came under review.

Prof. J. B. Harrison (British Guiana) then read a paper, "Notes on Sugar Cane Experiments," the joint production of Mr. G. S. Jenman and himself, followed by one, entitled "Past and Future Work in Sugar Cane Manurial Experiments," by Prof. d'Albuquerque (Barbados). Both papers covered somewhat the same ground, and were followed by a long discussion. It was generally agreed that nitrogen is the constituent of cane manures which chiefly governs the yield; but the experiments upon the use of phosphatic manures have been contradictory in different places. The application of potash and lime to cane fields gives profitable results in soils where these constituents of plant food are deficient. A discussion of considerable length took place upon the best and most economical way to conserve and utilise the nitrogen and mineral constituents of plant food in farmyard manure; and the desirability of extended trial of leguminous green dressing was urged upon West Indian planters.

Prof. d'Albuquerque, in the next paper, explained "A Method of using Control Plots in Experimental Field Cultivation." The method, which is only applicable where small plots of, say, one-thirtieth of an acre are used, partly consists in weighing the crops from a number of small no-manure (control) plots not far apart, and calculating the no-manure yield of the intervening plots on the assumption that in a uniform field the change of fertility is continuous from one control plot to the near next one. The other part of the method depends upon the manipulation of the figures obtained from the manured plots in relation to the calculated no-manure yields, and the interpretation of results.

The same author in the next paper, "The Possibility of Improving the Sugar Cane (*a*) by Artificial Cross-fertilisation, (*b*) by Chemical Selection of 'Seed Cane,'" under the first head argued that crossing different varieties would lead to the production of canes possessing desirable characters derived from both parents, and detailed some methods by which systematic experiments should be carried out. The second part of the paper dealt with the feasibility of increasing the sugar productiveness of a given variety of sugar-cane by propagating it with tops cut from canes richer than the average of the variety; the practical difficulty is to find a sure test of an inherently rich cane, as opposed to a cane rich because it is riper or more favourably situated.